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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

SER. II, VOL. 6 JANUARY, 1944 No. 3

SECTION OF GEOLOGY AND MINERALOGY[1]

DECEMBER 6, 1943

PROFESSOR GLENN L. JEPSEN, Curator of Vertebrate Paleontology, Princeton University, Princeton, N. J.: *Phylogenetic Trees.*[2]

Some of the most difficult and elusive problems of phylogeny, the evolutionary history of organisms, are now being reattacked by a combination of techniques and information from paleontology and genetics. Neither of these disciplines, nor any one of the many other biological and geological sciences such as comparative anatomy, embryology, stratigraphy, and ecology, is wholly adequate for rational explanations of the complexities of evolution. Each, however, contributes essential data for a synthetic approach to fundamental questions about the rates and processes of evolution which, itself, has been called the "central fact" of biology and is the principal guide in geological chronology and the correlation of strata.

With the development of new opinions about the methods and speeds of evolution, the patterns of family trees which are popular as abbreviated and graphic interpretations of phylogenies have been modified. A quarter of a century ago the common or garden variety of phylogenetic tree was the familiar diagram which looked like an oak or a maple with a central trunk and spreading branches. Only the occa-

[1] No meetings were held in December by the Sections of Biology and Anthropology.
[2] At the time of its presentation, this paper was titled "Geology, Genes, and Genealogy."

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 6, No. 3, January, 1944.
This publication is distributed to Members and is published monthly from November to June, inclusive, at 109 West Chestnut Street, Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.
Executive Secretary: Eunice Thomas Miner.
Assistant Editor: Ethel J. Timonier.
Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

sional dashed line which, as the label said, indicated inference (and thus suggested that the solid lines were facts) prevented these trees from looking like rather poor botanic sketches. The growth process, unlike anything in nature, consisted of a series of grafts. If a limb withered in one position it was hacked off and attached somewhere else. Or, a tree could begin as a clone or cutting suspended in mid-air and develop in any one direction or in many.

Most paleontologists had their own nurseries of seedlings in their laboratories and their published groves of adult trees in their bibliographies. Nearly every publication date was Arbor Day. Naturalists almost came to regard these paper and ink trees as true works of nature, and became ardent conservationists with the unstated but rigidly practiced motto of "Woodman spare that tree!"

For the welfare of the phylogenetic woods as a whole there were probably too many of these trees and their patterns were too standardized, but occasional comments that the stand of good timber could be improved by logging through the whole forest were met by the suggestion that there were better places to use well-ground axes. In one or two notorious instances the trees grew so rapidly and then decayed at such a rate that the authors who had fabricated them found themselves out on limbs with no visible means of support. The resultant crashes served as warnings that genealogical trees for fossils must be strong enough to support the opinions they are devised to symbolize and that large branches shouldn't be inferred too readily from small morphologic evidences.

At this same time, in the early thirties, other factors contributed to an increasing scepticism about the accuracy of some of the pedigree diagrams. When geneticists suggested that these sketches had too few genes and chromosomes in their growth formulae, paleontologists, as alert forest wardens, immediately recognized this possibility and began to suspect that geneticists themselves would sooner or later learn to ascend phylogenies, microscope in hand, or even to grow them if rearrangements and mutations of genes were the seeds of all evolution. This belief, that geneticists by their ingenious manipulations of *Drosophila* were developing precise new techniques which would provide all of the answers for all of the problems of evolutionary sequences, had a curiously intimidating effect upon paleontologists. The "chemical accident" concept of alterations in the genal complex seemed to mean

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that structural changes were entirely fortuituous and non-adaptive in origin, and this appeared to be a blight upon the family trees.

What the paleontologists didn't know was that geneticists among themselves had many differences of opinion about the meaning of their own work and were, in general, too occupied with chromosome forms and numbers and gyrations in mitosis and meiosis to be very concerned about extrapolating these data into studies of evolution. Paleontologists tried, without much success, to interpret the significance of the short time of genetics experiments and the minuteness of viable mutations in terms of the millions of years and the profound morphological changes of phylogeny.

As the literature of genetics rapidly grew in volume, it also became increasingly esoteric. In the lack of a summary statement of its meaning by the geneticists themselves, paleontologists tried to translate it, learned some of the jargon of Mendelism and cautiously attempted to apply the words and concepts of paleontology.

But the new patois didn't encourage the growth of family trees. Even some of the old established groves looked a bit withered. Perhaps they needed trimming. Paleontologists, in their anxiety, founded the sceptical school of phylogenetic tree surgery, sharpened their erasers, and went to work. They did a thorough job. When they had finished pruning they reassembled the fagots into two general kinds of sketches. One kept the old oak pattern but consisted entirely of dashed lines and the other looked like the candlewood ocotillo with its twigs ascending almost parallel from the ground. No one cared or dared to draw a solid line from any one fossil group to any other. Paleontologists who worked with abundant material, like that provided by the rodents, composed the chant "Parallelism, parallelism, and still more parallelism."

When geneticists saw the new forest of family stalks, they concluded that paleontologists had revised the former pattern from specifications which were dictated entirely by the fossils themselves. This of course was not true, and, although the former growth had been too lush, it appears from the perspective of the present time that the conversion from tree to pole may have been too complete, that some sound trees were needlessly destroyed in anticipation of diseases which never developed. Paleontologists are discovering now that geneticists neither had a magic tree growth formula composed of cell nuclei nor had been particularly concerned about compounding such an elixir. Unfortunately, some paleontologists have concluded that genetics has little or

nothing to offer to paleontology, but many of the clan are pooling their techniques and data with those of the geneticists in an attempt to make some family trees which will satisfy the facts and the theories of phylogeny. Successful results will have far reaching effects in biology and geology.

The participants of this common effort are finding that their motives and methods have some close similarities and some considerable differences, and that their general conclusions are in harmony. Geneticists can advance these studies by transferring more of their attention from gene mutations to structure and paleontologists can offer case histories and time to test some of the theories of genetics. Already, many notions about orthogenesis and irreversibility and other concepts are being modified by the confluence of evidence from the two sources. We are beginning to see why some of our former discussions about the relationships of environment and structure and function were futile, because these factors now appear to be inseparably interlocked in natural totality.

Genetics is essentially an experimental science, paleontology is historical. They join in trying to "predict" what has already happened. There are many difficulties, in addition to the differences in jargon, which work against complete mutual understanding between specialists in the two disciplines, but these do not appear insurmountable. For a while, there was a tendency to assume that genetics, or at least that part of the subject which can be applied to paleontology, was far simpler than it is, that, if a mechanism of inheritance were discovered in one group of organisms, it could be applied to all plants and animals whether they consist of one or of many cells.

Paleontologists have been unable to pace the literature of genetics from the time it raced beyond primary Mendelian inheritance and the simple dominant-recessive allel relationship. They have been puzzled about the degree to which laboratory materials in controlled and artificially stimulated short-term experiments resemble natural situations. Geneticists, too, of course, have been interested in this problem and have hunted and discovered instances where mutation rates and kinds in nature are quite similar to those which occur in bottle cultures.

Many data of paleontology can become increasingly significant and less receptive to geologic abuse as their interpretation is aided by genetics. Attempts to correlate the temporal and geographic and somatic features of fossils with genetic principles are highly involved en-

terprises however, with no obvious routes to success. It is desirable, in studying fossils, to be able to discriminate between structures which may be treated as inheritance factors or units (regardless of whether they were actually controlled by one or many or no single genes) and those which were individual developmental modifications.

Direct tests for distinguishing genotypic from phenotypic variations in living forms are largely experimental, but there are numerous possibilities of interpreting the structures of fossils as exemplifying one or the other. The hypostyle on the teeth of early Oligocene horses once seemed to be an individual phenotypic variation whose presence or absence was controlled by ontogenetic accidents. Now it seems possible to interpret the cusp as a relatively simple gene mutation, probably dominant, and to trace its diffusion into and through populations of *Mesohippus* and beyond, for forty million years to the present time.

Serious errors in stratigraphic correlations can result, if the hypostyle is regarded as an important or determining criterion for the separation of species or genera, but, if it is recognized as the manifestation of a drifting gene, the possibilities of misusing it in geological work are reduced. This appears to be a double-barreled paradox for, while we decrease the significance of the hypostyle as a morphologic and systematic item, we simultaneously increase its geologic and genetic values and its resistance to taxonomic treatment.

Geneticists, like most neontologists, are handicapped in attempting to help the paleontologist, because they lack experience with the techniques of establishing concepts of absolute and relative geological time, and with the colossal number of fossils and of kinds of fossils that have been collected and studied. Partly for these reasons neontologists fail to understand the concern of paleontologists about the fact that our system of nomenclature names stages of a process, elements of a continuum, and yet has no simple way of indicating their temporal or structural positions. It is unfortunate in taxonomy that categorical names do not evolve, although the nominees continuously do so. This inadequacy of our language symbolism is a serious but not an unusual situation. A developing science freqently inherits its first vocabular wardrobe from older relatives and we patch and let hems out until too many of the seams split before we make clothes that fit.

Not only tooth cusps but other structures of fossil horses have been and can be still further utilized for projection of genetic tests, because horses are among the most numerous of fossil vertebrates and

their evolutionary history is well known for most of the Tertiary. Using low figures for numbers of individuals and generations, it has been calculated that if all of the horses that have lived from *Eohippus* (or, *Hyracotherium*) to *Equus* were to form a parade seventy-five miles wide and to walk steadily, as close together as possible, at six miles per hour, they would require ninety years to pass the reviewing stand where the taxonomists would be going mad trying to establish dividing lines, where none existed in the equine spectrum, between species or between genera.

A pile of photographs, one of each horse, on paper of the thickness on which this is printed, would be about ten million miles high. No one knows how many thousands of fossilized specimens have reached museums, but certainly only one horse carcass in several million has survived the hazards of burial, fossilization, solution in the rock, removal by erosion, and the chances of discovery. For other groups, such as the early Tertiary primates, the museum expectation is probably much lower, perhaps on the order of one chance in ten to the tenth or fifteenth power.

Attempts to correlate rock thicknesses with degree of morphologic differences have sometimes led to irresponsible statements that one organism could not have been descended from some particular earlier form because the time interval between them was too short for such changes to occur. This interpretation is of course legitimate for some situations, and the principle involved in it has other important applications, but there are many cases where conclusions based upon it will be dubious until we know more than we do now about rates of evolution.

Concepts about "index fossils" can be clarified and their utility in correlation of strata improved by more attention to principles of evolution and genetics. We have all been told that the best index fossils have short time ranges and wide space distribution. "Short time range" frequently means that a certain kind of fossil has been found only in narrowly limited zones. A primary knowledge of genetics and paleontology might have absorbed or prevented some of the shocks that geologists have had upon learning that the geologic position of certain forms was controlled by ecologic or facies factors and had unpredictable temporal significance. Correlating strata by the presence in them of organisms of unknown past and future is a dangerous and treacherous practice.

With the techniques and materials now available in genetics and

paleontology, we can make further progress in resolving some of the intriguing puzzles of phylogeny. Although, at first thought, there would appear to be nothing more difficult than an estimate of the genetic make-up and environments of a fossil which has no genes and whose milieu is concealed by rock and time, nevertheless, the tantalizing residues suggest many research topics. It might be possible even to detect a gene creeping across a Paleozoic land or water bridge among a population which was itself sedentary; or to observe the mating of a pair of alleles in the Mesozoic; or to follow the ascension of a mutation through several thousand feet of Tertiary sediments; or to infer that some Horatian gene kept an ancient bridge against certain groups, but not others. Whatever a gene is, it may be one of the most durable matters in nature.

To some of the following questions which are pertinent to the specifications for family trees, we already have incomplete answers from genetics and paleontology as well as from other sources, but further documentation and integration of the topics is desirable.

(1) Is there a general or normal cruising speed of evolution? Would the average graph of morphological change in a group or phylum (in the linear sense), plotted against time, be a straight line?

(2) To what degree does the slope of such a curve depend upon population structure and upon distributional factors? Do small numbers of inhabitants of limited insular areas consistently transmute more rapidly than large continental groups?

(3) Are rate fluctuations within an evolving phylum concomitant or correlative with regional geologic or worldwide cosmic events, or with accelerations and decelerations of other groups?

(4) Are there times of extreme variability near the origin or the decline of a group?

(5) Is there any correlation between variability of organism and geologic span of its existence?

(6) Does a group dichotomize by the assumption of differential rates of evolution by population fractions? If so, what is the nature and duration of the anastomosis or inosculation while discontinuity is being established? What genetic and nongenetic factors can cause discontinuity?

(7) For how long and to what extent is reversibility an actual pattern?

(8) If homologous mutation is an explanation of morphologic

parallelism, what causes the phenomenon? To what extent is gene mutation non-random?

(9) What are the genetic mechanisms which have produced the numerous instances of convergence where groups of organisms develop genetic diversity and morphologic similarity? Can we establish any principles with high predictive value from these examples?

(10) Is linkage a common factor in the cases which have been called overspecialization and extinction and orthogenesis? How many historical examples do we have of differential rates of mutation of contrasting morphological features which are genetically linked? Is it possible for one quality or structure to mutate toward optimum adaptive development through orthoselection and at the same time induce or control other mutations which are progressively inadaptive or lethal? Are the antlers of the Irish elk, a sex-limited structure, an example of some such type of unfavorable carrier linkage, a kind of genetic suicide pact?

One additional question that has caused much debate between and among geneticists and paleontologists is "What causes the dearth of connecting or integraded forms between the major kinds of organisms?" The answer, apparently, is that the question is premised partly upon an overestimate of the number of gaps in the record, and that there may be several reasons for our failure to find and identify more intergrades than we actually do. Some geneticists and paleontologists have exaggerated the missing-link concept and have proposed enormous systematic mutations (*macrogenesis*) as the explanation. Statements like the half-serious quip that "The first bird hatched from a reptile's egg" emphasize the suggestion that the record gaps occur at the most critical points in phylogeny, between the divergent dashed lines of the family tree.

There are, in general, three kinds of intergrades: (1) a section of a branch, (2) a Y or a fork where a branch divided into two of about equal size, and (3) the origin of a minor limb from the main trunk. Paleontology can sometimes, but not always, distinguish between the latter two because, as with natural trees, a forked seedling may develop into a twin tree with trunks of about equal size, or either branch may become the larger one, or their size relationship to each other may be reversed one or many times.

All members save the terminal ones of any single evolutionary continuum are intergrades of the first kind and these are, of course,

well known and recognized, but on the diagrams little interdash spaces are frequently placed between them, to receive subsequently discovered dashes, or just in the name of caution.

The second sort of intergrade is in the region of a dichotomy. Vertebrate paleontology has apparent examples of organisms at or close to the point of branching of the Y pattern for every two diverged classes from fish to mammal and to bird, between crossopt and amphibian, between amphibian and reptile, between reptile and mammal, and between reptile and bird. These forms have the great merit of occurring at about the proper times, so that they are not as anachronistic as many "structural ancestors" (those morphological treasures that lived too late).

Many Y cases could be cited at all categorical ranks. These are not nearly as rare as they are held to be in popular scientific opinion. Indeed, in numerous instances where strong evidence suggests that two groups dichotomized from one, we have more than one candidate for the ancestral position and can't get a quorum to legalize the election. This admission is probably revealing, for, to change the metaphor, when we tug at the cloak of knowledge to cover one area of ignorance, we expose another to the chilly drafts of inquiry.

After the two "good" genera of saber-toothed cats, *Hoplophoneus* and *Eusmilus*, were described and named, a third form, a new species, was recovered from older rocks. It constitutes an admirable ancestor for both *Hoplophoneus* and *Eusmilus*. Clear as the concept may be that this is a case of the origin of two species from one, taxonomy makes the divergence appear to be generic in one of the directions. Morphologically, the new type resembles members of species of the other two genera, about equally, and in the absence of either genus would be placed in a new species of the other. To manufacture another genus would be incompatible with the calibre of generic grouping among the felids. This dilemma ended, but without solution, when the new species was allocated to one of the already established genera, *Hoplophoneus*. In the original description, this puzzling situation was noted, with the comment that the disjunction and recombination of the roots of the two generic names *Hoplophoneus* and *Eusmilus* would have provided a word, "hoplosmilus," which might suggest the morphological relationships of the case. A foreign author facetiously replied that the reciprocal root-combination pleased him more, being "euphoneus."

A yet-undescribed bat from the early Eocene, possibly the oldest known representative of the order, illustrates a Y case involving suborders. The two suborders of modern bats can be characterized by two very obvious structures. Megachiroptera, the large frugivorous tropical forms, have a claw on the index finger, and have simple teeth, whereas the Microchiroptera lack claws but have teeth of a complex form. The new fossil shows the claw of the Megachiroptera and the tooth-form of the Microchiroptera and, so far as the evidence is known at present, could have been ancestral to either or both of the living suborders. It is to be treated as a member of a new suborder.

There are also numerous examples of the third kind of intergrade:—at the place where the twig issues from the main trunk, the small population evolves divergently from the common herd. Two examples of highly specialized mammals may be cited. *Galeopithecus*, the doubly misnamed "flying lemur," is the sole living form in the order Dermoptera, and *Orycteropus*, the "earth pig," has a similar distinction in the order Tubulidentata. The former is insularly distributed in the Philippine Island area, and the latter lives in Africa. Both may be regarded as examples of small populations, although individuals are locally numerous, and this situation may have endured since the early Tertiary.

The incisor teeth of the flying lemur are nature's patent on the comb, an extraordinary dental structure the function of which is still debated by naturalists. *Plagiomene*, from the Wasatchian Eocene, had an incipient development toward this specialty and, in other almost equally remarkable ways, forms an intergrade about midway from a generalized insectivore to *Galeopithecus*.

The strong legs and peculiar pulpless teeth of *Orycterops* provide the means for digging and eating termites, almost its only food. Instead of lacking primeval relatives which foreshadowed its peculiarities, this genus has been provided with two claimants by paleontologists. Both are of early Tertiary age, *Ectoconus* from the Paleocene, and *Tubulodon*, the rare Wind River Eocene form. This case will remain a taxonomic impasse until further evidence is discovered, but it illustrates how an intergrade may be nominally missing, yet possibly present. Taxonomists cannot decide which is the more likely ancestor and hence place neither in the Tubulidentata, and the order thus appears on the records as having no ancient forebears.

These instances illustrate some of the ways that no-longer-missing

links may be concealed in the literature and camouflaged by systematics, but the principle evolutionary speculation of macrogenesis, that the major genetic and morphologic advances occurred in single large steps, has such great significance to paleontology and stratigraphy that it should be very carefully considered. If catastrophic structural changes in linear phyla were a common or actual mode of evolution, we could not expect them to occur in all lines simultaneously, and yet, in many instances in the stratigraphic record, this might appear to be and has been cited as the case. Locally, the organisms above a certain horizon in a continuous rock sequence may differ considerably from all related forms found at adjacent lower levels, but, in every case which can be illustrated by reasonably complete evidence, the explanation is hiatus, non-deposition, a gap in the temporal and sedimentary record as well as in the phylogenetic sequence. The missing records have so frequently turned up elsewhere, only slightly worse for wear, that we can use the degree of morphologic change during a hiatus as a rough guide to estimating its span. Actual fluctuations in evolutionary rates appear to be local in terms of place, time, and biologic group.

The evidence for *microgenesis,* or comparatively slow evolution by minute changes, increments and decrements, seems to be good. Many genetics experiments indicate that the genetic complex of animals tolerates only mutations of relatively small morphologic expression, that the lethal threshold is low. Paleontologic samples of many phyla show an inverse relationship between the completeness of the record and the morphologic difference from one group to the next. However, even if microgenesis is as general a pattern of evolution as it appears to be, there are still many factors which might prevent our discovery or recognition of intergrades, particularly of those in the most critical positions in phylogeny.

These factors, some of which have little or no evidence to support them, may be divided into two assumptions: (1) that no record survives, and (2) that the fossils were preserved but are rarely found. Under the first might be listed the suggestions: (a) that the tropics, where even resistant hard parts quickly decay or are otherwise rapidly destroyed, were the "cradle of evolution"; (b) that geocratic times, when abnormally large areas were land, or diastrophic periods, when little or no sediment was deposited (and hence opportunities for preservation were at a minimum), are to be correlated with maximum evolution rate; (c) that the fossils were removed by solution after they

had been buried; and (d) that they have been destroyed by erosion during and following disastrophism. It is difficult to understand how the latter two factors could act selectively upon most intergrades, but a number of rationales, which will not be repeated here, have been advanced.

There are several explanations for the second assumption; namely, that although fossils of intergrades were preserved, they do not occur with average frequency and hence have fewer chances than other forms of being discovered. Some of these postulates about intergrades are (a) that they lived in areas which have not been thoroughly explored, such as tropical or insular regions, (b) that they existed in relatively small communities, and (c) that, although they evolved by microgenetic changes which were qualitatively and quantitatively average, the rate at which these mutations occurred or achieved morphologic expression was accelerated.

These two latter suggestions are worth particular consideration, for it is possible to interpret much evidence from genetics and neontology and paleontology as supporting them. Paleontology contributes the fact that many apparent Y intergrades are represented by only one fossil specimen or by a few. This suggests that these organisms lived in small communities or were thinly distributed over wider areas. Genetics not only supplies mechanisms that cause somatic innovations, but also provides theoretical means of increasing mutation rates among evolutionary novelties. The effects of rate genes and the release of accumulations of clandestine or cryptic mutations to viable morphologic expression may be accelerated when organisms are out of adjustment with their environment. There are also explanations from genetics and ecology of ways that this imbalance in the sphere of organisms and their surroundings may be accentuated and then gradually ameliorated.

These and other clues to phylogeny and to the shapes of family trees can be pursued by combining the information from genetics and from paleontology. As Pope expressed it,

> "T'is education forms the common mind:
> Just as the twig is bent the tree's inclined."

SECTION OF PSYCHOLOGY

DECEMBER 20, 1943

DOCTOR MARGARET MEAD, Associate Curator of Anthropology, The American Museum of Natural History, New York, N. Y.: *Cultural Approach to Personality.* Anthropological Comment on the Frame of Reference of Andras Angyal. (Doctor Meal served as substitute speaker for Mr. Geoffrey Gorer, whose absence was necessitated by war service.)

This paper is presented on the assumption that it is one of a series of discussions based on Dr. Angyal's frame of reference, as outlined by him in the opening paper,[1] as a résumé of the thesis developed at greater length in his book entitled, "Foundations for a Science of Personality."[2] In this discussion, I shall limit myself to Chapters I to VI of that work, giving special attention (1) to Dr. Angyal's general frame of reference and its usefulness for providing a meeting place for social anthropologists and other students of personality and (2) especially the specific treatment of culture elaborated in Chapter VI.

Dr. Angyal's use of the term "biosphere," and his insistence upon the usefulness of not singling out organism from environment, but upon regarding the organism and its biologically relevant environment as a whole, is completely congruent with anthropological method, noting that the latter can then appropriately be analyzed either into features, abstractions, or holistic units, "according to its structural articulation." The term, "a culture" (as compared with the term "culture," as used by anthropologists), involves the use of these two sorts of units. Therefore, such abstractions as "ethos" and "eidos" are the types of deutero-learning, as used by Gregory Bateson,[3] the type of abstractions which may be based upon Erik Homburger's graphic presentation of types of modal behavior,[4] or the use which could be made of L. K. Frank's conception of time perspective.[5] In all of these cases, a series of "cultures"

[1] Angyal, Andras. Basic sources of humean motivation. Trans. N. Y. Acad. of Sci. Ser 2, 6 (1): 5–13. 1943.

[2] Angyal, Andras. "Foundations for a science of personality." Commonwealth Fund. New York. 1941.

[3] Bateson, Gregory. "Naven." The University Press. Cambridge, England. 286 pp. 1936.
Bateson, Gregory. "Social planning and the concept of 'deutero-learning.'" Science, Philosophy and Religion. Second Symposium. Pp. 81–97. New York. 1942.
Bateson, Gregory. "Some systematic approaches to the study of culture and personality." Character and Personality. 6: (1). 1942.

[4] Homburger, Erik. (Erikson, E. Homburger.) Configurations in play-clinical notes. Psychoanal. Quart. 6: 139–214. 1937.

[5] Frank, L. K. Time perspectives. Jour. of Soc. Phil. 293–312. 1939.

may be analyzed into an abstraction which may be conveniently applied to anyone of them. When, however, a culture is regarded as an unique whole, as is possible with isolated and homogeneous primitive societies, then it may be analyzed into units which are primarily relevant to that particular whole, and are not cross-cultural abstractions, but specific cultural holistic units. This approach is best represented by the work of Ruth Benedict,[6] and some of Dorothy Lee's papers.[7] My own work and that of Mr. Gorer,[8] who was to have presented this paper tonight, falls somewhat between the two approaches, using cross-cultural abstractions based upon the development process, but also invoking a number of holistic units specifically referred to the particular culture under description, or, in my own case, to the culture area under discussion.

As anthropologists have historically dealt with a group of people sharing a common culture, living in a given environment, they have also traditionally dealt with a biosphere that is included within their frame of reference, composed of both the biologically relevant environment and the organisms. "Tribe A uses neck rests" is a statement that has complete biospheric reference. It may be resolved into the following statements among others: "Individuals born into Tribe A will include, in their symbolic system, the idea neck rest. So, all objects, recognizable as neck rests, will be biologically relevant to them. Actual material neck rests will exist. The neck rests of other tribes, if encountered, will be perceived as neck rests and not merely as curiosities. Neck rests that do exist within the A biosphere will have a traditional form, and the members of tribe A will have habituated methods of making, or acquiring, neck rests of the traditional form. All individuals of normal intelligence within the A culture will react in terms of the existence of neck rests, whether they do or do not use neck rests, and whether they do or do not alter the traditional form of the A neck rest." Re-examination of this series of statements will demonstrate, I think, that any formal statement about a culture includes the idea that organisms who have been so moulded by their experience in a culture that certain types of response to objects, which are regularly within their environment, will themselves be regular. It is important to emphasize here that the

[6] Benedict, Ruth. "Patterns of culture." Houghton Mifflin & Co. New York. 1934.
[7] Lee, Dorothy. A primitive system of values. Jour. Phil. of Science. 7: 355–378. 1940.
[8] Gorer, Geoffrey. "Himalayan village." Michael Joseph, Ltd. London. 510 pp. 1938.
Gorer, Geoffrey. Themes in Japanese culture. Trans. N. Y. Acad. Sci. Ser. 2, 5 (5): 106–124. 1943.

social anthropologist deals with regularities, not with uniformities.[9] He is not merely concerned with the number of people who do or do not use neck rests, but with the fact that in a neck-rest using culture, both use and non-use will include neck-rest reference. The extent to which a given number or proportion of the individuals who share that culture at a given time do use neck rests will be one of the holistic comments which can be made upon that culture, but would have to be reduced to a more abstract form, for example: "degree of actual participation of every individual in the culture in a common form of domestic furniture," to be discussed cross-culturally. Confusion between statistical statements of uniformity and statements of regularity are responsible for some of the current misunderstandings of the relationship between the study of culture and the problem of sampling. The fact that every individual in a culture is himself a representative of the regularities of the culture, although he may display a minimum of the usual overt behavior commonly described as characteristic of the culture, is usually omitted. In a crude paraphrase, when other students of personality attempt to include the concept of culture within their frame of reference, they regard using neck rests as culturally determined, but the individual who refuses to use a neck rest would be classified as behaving, in a personal or individual manner, as if he were in some way distinct from the culture. However, the individual who refuses to use a neck rest, and the individual who insists that a neck rest is really a weapon for hitting his mother-in-law, would also be behaving in cultural terms, not *out* of them, for the original inclusion of neck rest within the biosphere would be part of themselves also.

This confusion between cultural experience and personal experience, in which the personal experience of life is seen, not as something wholly within the terms of the culture, but in some way different and beside it, recurs in Dr. Angyal's discussion. He speaks of the content of an object as being "in part culturally defined and in part built up under the influence of personal experience" (p. 162). This name confusion is evident when he speaks of the channels of dynamic expression as "built up as a result of personal experience and under the *influence* of socio-cultural *influences*" (italics mine). Such statements suggest that an individual has, in the course of his life, cultural experience,—he sleeps on a neck rest; and personal experience,—he sleeps on a particu-

[9] Mead, Margaret. "Educative effects of social environment as disclosed by studies of primitive societies." Environment and Education: a Symposium. The University of Chicago. No. 54: 48–61. March, 1942.

lar neck rest which has a serpent on it, and has decided he didn't like neck rests. The same lack of systematic inclusion of the concept of culture is manifest in the statement that an individual has to adjust to a culture, that it is "a complex of environmental factors to which one has to adjust; and second, it is assimilated by the individual and becomes an important part of the personality" (p. 206). The first statement postulates culture as something outside the organism; the second as something inside. But, from the anthropological point of view, the individual organism develops *within* the culture from the moment of birth, and, in all probability (although the material to document this statement is not yet available), from the beginning of gestation. The culture of those individuals who form his social environment is built into his body, into its tempo and its rhythms, into its sensitivities and its insensitivities. The reality of culture permeates the organism, and at no time can a human being, bred in a group of human beings, be said to stand outside culture, or to have personal experiences that are not culturally shaped. Such a statement as "hunger becomes appetite" is only relatively true, for even the hunger of the three-day old baby is already patterned by the feeding habits of its culture: whether a newly delivered mother gives her baby the breast; whether a child is fed intermittently but almost continuously; whether it is starved for three days, etc. Dr. Angyal uses the phrasing "cultural determinations are superimposed also on primary physical functions such as sex, food intake and excretory functions" (p. 187), but, as none of these physical functions are ever manifested by a human being who has not been at least prenatally moulded by contacts with other human beings, who themselves embody regularly patterned forms of behavior, we can not speak of culture being *superimposed* upon, but must speak rather of physical functions which develop in a culturally patterned way. A human being developing without contact with other human beings (if, for instance, it were possible for him to be reared by another mammal) would provide valuable data on original nature. But it is untenable to consider any human being, who have been reared among human beings, as having at any point in his life-history, or in any layer of his personality, or in any segment of his soma, some area somehow unpatterned and set vis-à-vis culture.[10]

The anthropological approach to the problem of personality means, in effect, that the anthropologist's task is to describe this over-all cul-

[10] See question raised by Dr. Hans Fried in discussion.

tural setting, within which any given set of individuals, or a given individual, develops, and to identify and describe the process in which the individual expresses the regularities of the culture of which he is a member.[11] My original illustration of neck rests was at the very simplest level, a habit connected with an object of material culture. But, at each level of description, the anthropologist's task is to describe the way in which the culture must be taken into account. The term "personal experience" raises a problem of a more abstract level. Before the student of personality can study the effect of "personal experience," it is necessary for the anthropologist to say what order of personal experience individuals born within that culture can have. Travel, for instance, experience of another culture, may be something which members of one culture will be able to integrate and assimilate, deal with consciously, and thoroughly symbolize (in Angyal's sense), while, for members of another culture, it may merely involve a disorienting experience of completely unassimilable sensations, objects which can not be named or classified, or even subsequently remembered. For an American, who goes for a walk in the country and hears a stone dislodged from the bank along which he is walking, there is almost no chance that if the stone is too small to be a presage of accident of any sort, that it will become a "personal experience." For the Plains Indian, the same incident might become the basis of a "vision" which would provide a lifelong validation of his social position in his community. Attempts to collect life-histories from members of different cultures immediately reveal the most conspicuous discrepancies in the extent to which the idiosyncratic details of an individual's life have become personal experience, or have been, under a different cultural pattern, smoothed back into a stereotyped position. Iatmul natives will quite readily relate the story of their lives, fill them with incident and conversation which have all the ear marks of remembered events to which the individual personality might have been expected to have responded, but then continue the story, without a break, into some event where they were not present, or into a culturally stereotyped future. So an unmarried boy of eighteen may relate his marriage and the birth of his children. Balinese natives, on the other hand, born and patterned by a culture which treats time as circular, and lacking effective devices for handling temporal sequences of any length, will relate series of episodes, some highly stereotyped, some with an idiosyncratic flavor,

[11] Bateson, Gregory. *Op. cit.*

but without time sequence. In other words, personal experience takes place with reference to an organism whose capacity for personal experience is already in that organism, both in terms of idiosyncratic endowments of sensitivity and "intelligence," and in terms of the whole modification which the organism has undergone as a member of a culture. The most personal, the most idiosyncratic experience that any individual can ever have, must be seen, for full understanding, as occurring within an acculturated person. It is not a question of thinking sometimes about the cultural aspect of a situation and sometimes of the personal, or sometimes of family culture and sometimes of wider culture, but of operating with the widest frame of reference always in mind. The anthropologist's task is, therefore, to provide this necessary widest frame of reference, to describe the relevant regularities which the student of personality will wish to take into account. This means that the anthropologist will collect "personal experience" as data upon the cultural regularities in terms of which events of an individual's life may be experienced, not as data about the personality of Ombomb of Aliton, or Nang Karma of Bajoeng Gedé, nor initially in order to make abstractions about the growth of personality which will transcend cultural lines. The anthropologist's first task is to organize material about individual versions of the culture so as to throw maximum light upon the culture, and the type of personality development possible, or probable, within that culture. But, with a series of cultures so studied and so analyzed, the anthropologist is then in a position to contribute materials and hypotheses upon forms of personality development, always, however, with his primary point of reference, the culture, shared by a number of members, rather than by the individual sharer of the culture.

There is, however, a still more abstract level at which the anthropologist can contribute to an attempt such as Dr. Angyal's, that is, to treat Dr. Angyal's whole statement as a cultural product and attempt to identify the way in which Dr. Angyal's hypotheses are themselves the product of his culture, and to what extent they are limited by the culture. When this is done, Dr. Angyal's basic antithesis between the trend toward autonomy and the trend toward homonomy will be seen to be characteristic formulations within Western European culture, formulations which are not inherent in the nature of life, or in the nature of man-in-culture, but simply part of our cultural approach to the world. Not only is this antithesis, which is a rewording of older forms of antithesis between man and society, one which individuals reared in our

own culture tend to create, but the particular form in which he expresses it, in which the social tendencies are subsumed under a tendency to submit, to passively become part of a "super-individual unit," is relevant to Euro-American culture at the present time,—to totalitarian theories of the state, and to the whole phenomena of fascism in its position counterposed to individualism, itself conceived as an extension of, or expension of, individual power over the environment. Such a formulation grows naturally out of a culture where goal-oriented activity and emphasis upon power over persons and things, including one's own life, is vigorously symbolized. However, in cultures like the Balinese, such a formulation would be irrelevant and non-descriptive of Balinese behavior.[12] Another of Dr. Angyal's statements, that "The organization of the attitudes of a person is essentially a hierarchical one" (p. 143), reflects our own cultural pattern, but would not be relevant to Iatmul culture, in which the concept of hierarchy, either in overt social forms or the corresponding character organization, is lacking.[13]

This question of the importance of understanding the extent to which hypotheses are culturally limited or restricted, and the related problem of how much they owe, for their richness and insight, to any particular culture, is one of very special importance for all of those sciences which deal with human beings, in which the observer *must* be included inside the experiment. While in the physical sciences, it is possible to work directly to hypotheses which, when verified, may be used, at either the theoretical or applied level, by members of other cultures than that in which the original work was done, the question of borrowing scientific hypotheses in the human sciences becomes much more complicated. It is probable that only after we have developed a series of culturally limited and culturally rich hypotheses in a number of cultural settings will we have enough material actually to build a system which is genuinely cross-cultural and includes, or constructively excludes, the many different cultural frameworks now present in the world. In the meantime, it is a function of the anthropologist to submit such frameworks as are developed to cross-cultural analysis and to point out the extent to which such cultural limitations and enrichments do occur.

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[12] Bateson, Gregory. The frustration-aggression hypothesis and culture. Psychological Review 48 (4): 350–355. 1941.

Bateson, Gregory. "Balinese Character: a photographic analysis." Bateson and Mead. Special publication of The New York Academy of Sciences. 2: 277 pp. December, 1942.

[13] Mead, Margaret. Administrative contributions to democratic character formation at the adolescent level. Jour. Nat. Asso. of Deans of Women. 4(2): 51–57. 1941.

Discussion

Dr. Ernst Kris emphasized the contribution which anthropological studies should be able to make to the problem of the limits of human variability. Other points discussed included a problem, raised by Dr. Hans Fried, that if we rejected Dr. Angyal's category of "adjustment to culture," we rejected an important point of leverage for social change, as, for instance, in German assumptions about military training which postulated a human unwillingness to be cruel which had to be overcome by training. Dr. Mead replied that, where a culture included in its phrasing of human nature the assumption that part or all of the cultural behavior was antithetical to human beings' natural tendencies, then such a phrasing did give us a valuable point of leverage for cultural change. Such an idea of "adjustment," however, was not a cross-cultural abstraction. Another questioner asked how it was possible to use the concept of culture, as based on the study of homogeneous, relatively static cultures, under modern world conditions, where husband and wife might come from radically different cultures and rear their children in a third. Dr. Mead replied that the behavior of such people would be no less systematically related to their experience, but that a higher level of abstraction, one defining the degree of heterogeneity, would be necessary. She cited the lack of expressive movement among Americans as a possible manifestation of heterogeneity, growing out of the handling which children receive when reared by representatives of several different cultures. A misunderstanding was cleared up about the need for developing anthropologists from each culture to study their own culture. Dr. Mead explained that she meant that we needed the culturally enriched contributions to scientific theory, not the specific observations of anthropologists on their own cultures. A question was raised from neurology, as to whether the science of culture and personality had not now reached a period analogous to the period in medicine when symptoms, on the one hand, and the nature of the bacteriological environment, on the other, were both known, but a knowledge of the dynamic mechanisms was still lacking. Dr. Mead replied that anthropologists approached the problem of dynamics through a study of the growth of the child within a culture, which was her usual method of approach, but that Dr. Angyal's presentation had not evoked discussion of this method. Mr. L. K. Frank, as chairman, brought out two points: (1) A culture may be viewed somewhat like the more recent

conception of a gas, as composed of a large number of molecules which only approximate to the pattern of behavior appropriate to their chemical composition, but, nevertheless, collectively give rise to the orderly, regular behavior we call chlorine or hydrogen. In a culture, we have the most frequent modal patterns of conduct, with deviations toward the criminal extreme and toward the psychotic extreme. Individuals vary in their conformity to the presented or modal patterns of a culture, according to their constitutional-temperamental readiness for such patterns, and, also, according to the degree and kind of culturizing tuition to which each has been subject. (2) In an intact, homogeneous culture, we have a unimodal distribution of conduct. In a culture that is becoming diversified and heterogeneous, such as ours today, we have a multi-modal culture. The question, then, is: What does conformity mean in such a multi-modal culture and what criteria of "adjustment" should be used?

Dr. Angyal concluded the discussion by saying that this preliminary exploration of the literature on primitive cultures had not led him to believe that his basic hypotheses would have to be modified.

REPORT OF THE ANNUAL MEETING

DECEMBER 15, 1943

The 126th Annual Meeting of the Academy for the election of Officers, Fellows and Honorary Members, the presentation of reports and the transaction of other business was held at The American Museum of Natural History on the evening of Wednesday, December 15.

The Corresponding Secretary reported that there are now 44 Honorary Members and 1 Corresponding Member upon the rolls of the Academy. Four deaths were reported during the past year.

The Recording Secretary's report stated that, during this second year of war, the Academy has proved its fundamental stability and usefulness by the complete continuance of its progressive program with especial emphasis upon its conferences and other scientific meetings and through the active publication of their results in a series of notable documents.

That these results are appreciated by the members of the Academy is attested by their active support and even by an actual increase in the membership total during 1943. A noteworthy evidence of the interest of the members in the Academy activities has been the cordiality with which they now assume the responsibility for expansion of Academy membership.

The Academy has held 28 Regular Sectional Meetings, at which 28 stated important scientific papers were presented. This is in addition to two excellent addresses delivered on the occasion of the Annual Business Meeting.

Ten informal receptions were held under the auspices of the various Sections of the Academy.

Five two-day conferences on special subjects of research were held during the year. These meetings were attended by outstanding investigators in these fields. The titles of the conferences are as follows: Section of Biology, "Parasitic Diseases and American Participation in the War"; Section of Physics and Chemistry, "High Polymers," "Sulfonamides," "Magneto Chemistry"; Section of Psychology, "Psychosomatic Distrubances in Relation to Personnel Selection."

Four Honorary Members, 4 Life Members, 124 Active Members, 35 Associate Members and 10 Student Members were added to the rolls, of which 163 are now in good standing and 14 await qualification

through payment of dues. Thus, a total of 177 new Members were added during the year.

There are at present upon the rolls of the Academy 1,985 members, of whom 260 are Fellows. These include 1 Patron, 95 Life Members, 123 Sustaining Members, 1,089 Annual Active Members, 603 Associate Members, 29 Student Members, 44 Honorary Life Members and 1 Corresponding Member.

The Editor reported that, during the fiscal year of 1943, The Academy published 872 pages. Of this amount, 670 pages were published in the Annals and 202 pages in the Transactions. In addition to the articles already published, Articles 2, 3 and 4 of Volume 45 are being paged and Article 5 is in galley proof. These are all scheduled for publication before the end of this calendar year. The illustrations for Volume 45, Article 6, have been engraved, the manuscript of the text is now in the Editor's hands, and the completed article will appear early in 1944. The manuscript of Volume 12, Part 4, of the Porto Rico Survey, is also being revised by the Editor and will be printed early in 1944. This will complete Volume 12 of the Survey.

The detailed list of the publications is as follows:

ANNALS

Volume 43, Article 6—"Protein Hormones of the Pituitary Body," by H. B. van Dyke, Bacon F. Chow, Vincent du Vigneaud, H. L. Fevold, George W. Irving, Jr., C. N. H. Long, Theodore Shedlovsky, and Abraham White. Pages 253–426. Published February 26, 1943.
This completes Volume 43.

Volume 44, Article 1—"Boundary-Layer Problems in the Atmosphere and Ocean," by C. G. Rossby, B. Haurwitz, Benjamin Holzman, Woodrow C. Jacobs, A. A. Kalinske, Phillip Light, R. B. Montgomery, and H. U. Sverdrup. Pages 1–104. Published May 29, 1943.

Volume 44, Article 2—"Criteria for Vertebrate Subspecies, Species and Genera," by Charles M. Bogert, W. Frank Blair, Emmett Reid Dunn, E. Raymond Hall, Carl L. Hubbs, Ernst Mayr, and George Gaylord Simpson. Pages 105–188. Published June 8, 1943.

Volume 44, Article 3—"Parasitic Diseases and American Participation in the War," by Horace W. Stunkard, Lowell T. Coggeshall, Thomas T. Mackie, Robert Matheson, and Norman R. Stoll. Pages 189–262. Published September 30, 1943.

Volume 44, Article 4—"High Polymers," by Raymond M. Fuoss, J. Abere, W. O. Baker, Henry Eyring, John D. Ferry, Paul J. Flory, C. S. Fuller, G. Goldfinger, R. A. Harman, Maurice L. Huggins, H. M. Hulburt, H. Mark, H. Naidus, Charles C. Price, John Rehner, Jr., Robert Simha, and A. V. Tobolsky. Pages 263–444. Published November 12, 1943.

Volume 44, Article 5—"Sulfonamides," by Colin M. McLeod, Paul H. Bell, Henry Irving Kohn, J. S. Lockwood, Richard O. Roblin, Jr., James A. Shannon, and H. B. van Dyke. Pages 445–538. Published December 14, 1943.

Volume 44, Article 6—"Psychosomatic Disturbances in Relation to Personnel Selection," by Lawrence K. Frank, M. R. Harrower-Erickson, Lawrence S. Kubie, Gardner Murphy, Donal Sheehan, and Harold G. Wolff. Pages 539–624. To be published December 22, 1943.
This will complete Volume 44.

Volume 45, Article 1—"The Relation Between Centriole and Centromere in Atypical Spermatogenesis of Viviparid Snails," by Arthur W. Pollister and Priscilla F. Pollister. Pages 1–48. Published November 30, 1943.

Volume 45, Article 2—"Lycaenidae of the Antilles," by William P. Comstock and E. Irving Huntington. Pages 49–130. To be published December 29, 1943.

Volume 45, Article 3—"Stellar Dynamics," by S. Chandrasekhar. Pages 131–162. To be published December 31, 1943.

Volume 45, Article 4—"Studies on Fresh Water Bryozoa," by Mary D. Rogick. To be published December 31, 1943.

Volume 45, Article 5—"The Social Behavior of the Laughing Gull," by G. K. Noble and M. Wurm. To be published December 31, 1943.

Volume 45, Article 6—"Experimental Modification and Control of Molts and Changes of Coat-Color in Weasels by Controlled Lighting," by Thomas Hume Bissonnette and Earl Elmore Bailey. To be published early in 1944.

TRANSACTIONS

Series II, Volume 5, Nos. 1–8, consisting of 202 pages, was completed, printed and distributed each month from November, 1942 to June, 1943, inclusive.

In September, 1943, Doctor Wilbur G. Valentine, the able Editor of the Academy publications, was called into Government service, much to the regret of the Council, and Doctor Roy Waldo Miner was appointed Acting Editor by that body to carry on the editorial work from October 12th to the end of the year. Mrs. Ethel J. Timonier has acted as Assistant Editor.

The Librarian reported that, in response to requests and orders, 19,416 separate publications, and 1,909 complete volumes of Transactions, Series II, were distributed as follows: *Annals*, 13,068; *Scientific Survey of Porto Rico and the Virgin Islands*, 888; *Special Publications*, Volume I, "Climate and Evolution," 73; Volume II, "Balinese Character," 525; and *Transactions*, Series II, Volume 5, complete volumes, 1,909, and 4,862 single copies.

The Library of the Academy has received, from exchange institutions, 977 separate publications.

The Treasurer reported that The Academy has continued to maintain a sound financial status in spite of the conditions affecting similar institutions during the war period.

The total income received from all sources amounted to $25,429.16. This is an increase of $2,678.85 above that of 1942.

Through the hearty cooperation of its membership, the Academy added 177 new names to its rolls, thus adding a slight gain of $88.55 to our revenue from membership dues, making a total of $8,345.00 from this source.

Receipts from sales of publications amounted to $5,423.16, an increase of $2,317.50 over that of last year.

The Council of the Academy took advantage of an opportunity to sell a real estate property which had been held for many years and which had been carried at a considerable loss. This resulted in a reduction of the book value of the Academy's holdings, but relieved the income-producing investments to such a degree that instead of yielding 3.5% as in 1942, during 1943 they produced 5.5%, giving a substantial net increase from this source.

The book value of the Academy's property, as of November 30, 1943, now amounts to $84,878.61; of which $68,254.10 is in stocks and bonds; $895.04 is a mortgage bond; and, the remainder, $15,729.47, is represented by cash in bank, a portion of which results from the redemption of bonds and is awaiting reinvestment.

Following the usual custom, the books of the Treasurer were duly checked and balanced at the end of the fiscal year, November 30, 1943, and have been audited by the Finance Committee, as provided by the Constitution and By-Laws.

The property of the Academy was verified and the Treasurer's report examined and attested by the Finance Committee as of December 14, 1943.

The A. Cressy Morrison Prizes of two hundred dollars each, for the two most acceptable papers in any field of natural science, within the scope of the Academy and its Affiliated Societies, were awarded to the following papers,[1] entitled:

"The Pre-Iroquoian Occupations of New York State," by William Augustus Ritchie, Rochester Museum of Arts and Sciences, and "The

[1] Abstracts of these papers are included in this issue of the Transactions.

Distribution of the Salamanders of the Genus *Plethodon* in Eastern United States and Canada," by Arnold Grobman, University of Rochester.

The committee desires to congratulate the authors of these papers and to express its appreciation of this method of furthering the advancement of science.

The following members were elected to Fellowship:

John M. Arthur, Ph.D.
Hugh D. Auchincloss, M.D.
Charles O. Beckman, Ph.D.
Alexander W. Blain, M.D., M.S.
Aaron Bodansky, Ph.D.
Oscar Bodansky, Ph.D.
Richard M. Brickner, M.D.
Mary L. Caldwell, Ph.D.
Leslie A. Chambers, Ph.D.
Herbert C. Clark, M.D.
Kenneth S. Cole, Ph.D.
Cornelius J. Connolly, Ph.D.
William H. Cowley, Ph.D., LL.D.
Lyman C. Craig, Ph.D.
M. L. Crossley, Ph.D.
David Davidson, Ph.D.
E. F. Degering, Ph.D.
Rene Jules Dubos, Ph.D.
Gioacchino Failla, D. Sc.
Margaret C. Ferguson, Ph.D., Sc.D.
Erich Fromm, Ph.D.
Joseph T. Gregory, Ph.D.
Ernst A. Hauser, Ph.D.
Philip Bovier Hawk, Ph.D.
Edna Heidbreder, Ph.D.
Kenneth C. D. Hickman, Ph.D.
Charles L. Hoagland, M.D.
Edward J. Kampf, M.D.
Bostwick H. Ketchum, Ph.D.
James H. Kimball, Ph.D., D.Sc.
Herbert S. Langfeld, Ph.D.
Eugene J. Lorand, D.E. (Budapest)
Max Bernard Lurie, M.D.
Leland S. McClung, Ph.D.
Warren Sturgis McCulloch, M.D.
Walter Christian Michels, Ph.D.
Ralph S. Minor, Ph.D.
Stanley O. Morgan, Ph.D.
Walter M. Nielson, Ph.D.
Peter K. Olitsky, M.D.
Alwin M. Pappenheimer, Jr., Ph.D.
Edward G. Pickels, Ph.D.
Robert N. Pease, Ph.D.
Hortense Powdermaker, Ph.D.
Anne Roe, Ph.D.
Mary D. Rogick, Ph.D.
William T. Salter, M.D.
Julius M. Rogoff, Ph.G., Sc.D., M.D.
James Plummer Schooley, Ph.D.
Gotthold Steiner, Ph.D.
J. G. Phelps Stokes, M.D.
W. Duncan Strong, Ph.D.
Ernst Weber, Sc.D., Ph.D.
John Warren Williams, Ph.D.

Honorary Life Membership was conferred upon the following eminent scientists[1]:

Oswald Theodore Avery, Sc.D., M.D., LL.D., Bacteriology, Member, Rockefeller Institute for Medical Research, New York, N. Y.

Alexander Fleming, M.B., B.S. (London), F.R.C.S. (England), Bacteriology, discoverer of penicillin; Professor of Bacteriology, University of London, St. Mary's Hospital Medical School; Hunterian Professor and Arris and Gale Lecturer, Royal College of Surgeons.

Sir Frederick Gowland Hopkins, M.A. Cantab., M.B., D.Sc. (London), Hon. Sc.D., London, Oxford, Manchester, Dublin, Sheffield, Glasgow, Leeds, Reading and Harvard; LL.D., Birmingham, Aberdeen and St. Andrews; D.C.L., Durham; Hon. M.D., Louvain, Budapest, Lausanne; Biochemistry, Professor, University of Cambridge, England; Sir Williams Dunn Professor since 1921.

Alfred L. Kroeber, Ph.D., Anthropology, Director, Museum of Anthropology, University of California, Berkeley, California.

[1] The class of Honorary Life Membership of The New York Academy of Sciences is limited to fifty.

Th. Svedberg, Fil. Kand., Fil. Lit., Fil. Dr. (Uppsala); Dr. Chem. (Uppsala); Hon. Dr., Wisconsin, Uppsala, Harvard, Delaware, Oxford; Chemistry, Developer of the Svedberg Ultracentrifuge Apparatus, Professor and Head of Physical Chemistry Institute, University of Uppsala, Sweden.

Arne Tiselius, W.K., Fil. Mag., Fil. Lit., Fil. Dr., Dr. Chem. (Uppsala), Biochemistry, Application of electrophoresis to proteins; Professor of Biochemistry, University of Uppsala, Sweden.

The following officers were elected:

For President
DUNCAN A. MACINNES

For Vice-Presidents

G. MARSHALL KAY
ANNE ROE
HORTENSE POWDERMAKER
RALPH H. CHENEY
LEWIS G. LONGSWORTH
RAYMOND B. MONTGOMERY

For Recording Secretary
MICHAEL HEIDELBERGER

For Corresponding Secretary
H. HERBERT JOHNSON

For Treasurer
DONALD BELCHER

For Librarian
BARNUM BROWN

For Editor
ROY WALDO MINER

For Councilors (1944–1946)
WALTER H. BUCHER
VICTOR K. LAMER
THEODORE SHEDLOVSKY

For Finance Committee
HARDEN F. TAYLOR, *Chairman*
HARRY B. VAN DYKE
ADDISON WEBB

After the Business Meeting, the following program was presented:

Address by the Retiring President

"How Do Tapeworms of Herbivorous Animals Complete Their Life Cycles?"

by

HORACE W. STUNKARD

"The Arctic Mediterranean"[1]

by

VILHJALMUR STEFANSSON

[1] No abstract of this address was received.

THE NEW YORK ACADEMY OF SCIENCES

Address of the Retiring President[1] given at Annual Dinner and Meeting December 15, 1943

PROFESSOR HORACE W. STUNKARD, Department of Biology, New York University, New York, N. Y.: *How Do Tapeworms of Herbivorous Animals Complete Their Life Cycles?* (This address was illustrated by lantern slides.)

It has been said that Nature follows dark and devious paths her wonders to perform. The truth of that statement seems more and more apparent, the more we know of the intricacies of biological phenomena. Certain observers discover the wonders of Nature only in far-distant climes, but to the illumined mind and observant eye—they are all about us—right in our own gardens. At the annual meeting of the Academy six years ago, I reported on the life histories of certain digenetic trematodes, tracing the alternation of generations and the transfers of hosts that take place in these involved and complicated life cycles. The account had all the aspects of a series of fairy tales, but the stories were true in general outlines and in the details.

I wish tonight to explore with you other dark and devious paths followed by other parasitic worms in the completion of their life histories. The cestodes or tapeworms are common intestinal parasites of all classes of vertebrates. In their several hosts, from the most primitive fishes to human beings, they manifest much variation in form. Typically, the cestode in the intestine consists of a scolex or head with an attached string of proglottids or segments which constitutes the strobila. The scolex bears certain organs, suckers or hooks, or both, by which it attaches to the wall of the intestine. The strobila may be short and consist of only a few segments, but usually it attains considerable length, often ten feet or more, and is composed of hundreds or thousands of proglottids. The region near the head is a zone of proliferation and growth, where new segments are formed. As these segments become progressively older, they proceed backward in the chain and the oldest ones are at the terminal end where, in taenioid species, they become detached and pass out of the intestine. Somewhere near the middle of the strobila, each proglottid develops reproductive organs, testes and ovaries; eggs are produced and fertilized and

[1] Dr. Stunkard served as President of the Academy during the years 1942 and 1943.

then pass into a uterine sac. The reproductive organs then degenerate and disappear; the eggs begin to develop and the terminal proglottids are filled with mature eggs. The eggs are microscopic. In different species, they vary in size from 30 to 80 microns, and each one contains a motile, six-hooked larva, known as an onchosphere. These larvae are exceedingly small, averaging about 20 microns in diameter. Thus, in the intestine, the tapeworm has two forms of multiplication: one, asexual, by the production of new proglottids; and the other, sexual, with the production of eggs and their development to the first larval stage.

But this is only half of the story. The tapeworm leads a double life. The larvae, encased in the eggs, pass out from the intestine of the host, but, with the rarest of exceptions, they are unable to infect the same host-species again. They can live for long periods within the protective covering of the egg shells, and never leave the shells unless they are ingested by some other animal. When they are eaten by an animal which can serve as an intermediate host, they emerge from their shells, leave the intestine, undergo metamorphosis, and grow into second stage larvae, which are then infective for the original host. These larval stages occur either in the body cavities of invertebrate animals or in cystic vesicles attached to or embedded in the tissues of vertebrates. With rare exceptions (the hydatid cysts), multiplication does not take place in the intermediate host. The tapeworm simply transforms from a first to a second larval stage and an intermediate host serves primarily for the metamorphosis of the parasite to a stage where it is able to continue its development when introduced into the intestine of the original host. In the more primitive cestodes, those of lower vertebrates, there are usually three hosts in the life cycle, that is, first and second intermediate hosts which harbor successive larval stages, and the second intermediate host is eaten by the final host where the worms are liberated, attach themselves to the intestinal wall and become sexually mature.

Most of the species are large enough to catch the eye of any alert observer. They have been known since ancient times and Aristotle recorded both the sexually mature strobilate stages and the cystic bladder-worm stages; although at that time there was apparently no suspicion that these forms constitute successive stages which alternate with each other in the life cycle single species. The description of a large number of tapeworms led to the formulation of scheme of classification by Zeder (1800). This author called the vesicles "Blasenwür-

mer," and proposed the name *Cysticercus* for what we now know to be the larval stage of *Taenia hydatigena*, a tapeworm of the dog.

Gradually, it became apparent that there was some connection between these bladder-worms and tapeworms. Malpighi (1698) had discovered the scolex in the cysticercus of the pork tapeworm. Pallas (1760) and Goeze (1782) described the scolices in the cysts of other species. The latter author noted the agreement between the scolex of the cat tapeworm and the scolex of the bladder-worm found in the liver of the rat and mouse. In this species, the scolex begins to proliferate and produce proglottids while still in the cyst. Goeze (1782) had observed embryos in the eggs of the dog tapeworm, *Dipylidium caninum*. Von Siebold (1835) and Dujardin (1837) described the six-hooked onchospheres and their movements, but the relation between these larvae and the bladder-worms was still obscure. Von Siebold (1844) predicated that bladder-worms represent hydropsical, degenerate worms that had been introduced accidentally into abnormal hosts. Although he still held that the cysticercus was an abnormal or teratoid form, von Siebold (1850) recognized that cysticerci were derived from onchospheres by metamorphosis. He stated, p. 209, "Diese Embryone (onchospheres) müssen jedenfalls noch eine Metamorphose erleiden, durch welche sie erst eine dem Kopfende der Taenien oder Bothriocephalen entsprechende Gestalt bekommen." Steenstrup's (1842) studies had called attention to the alternation of generations in lower invertebrates; whereupon Blanchard (1847) and van Beneden (1849) suggested that the bladder-worms are normal stages in the life cycle of tapeworms. The first step in the final solution of the problem was made by Küchenmeister (1851, 1852) who fed bladder-worms (*Cysticercus pisiformis*) from the mesenteries of rabbits to dogs and obtained adult tapeworms, *Taenia serrata*. This experiment demonstrated that these two forms, previously regarded as distinct species, were really only different and successive stages in the life history of a single one. Shortly thereafter, van Beneden (1854) discovered the life cycle of *Taenia solium* of man. He fed eggs of the tapeworm to pigs and obtained the larvae, *Cysticercus cellulosae*, long known as the cause of "measly" pork. Following other successful experiments on the life cycles of cestodes of dogs and cats, Leuckart (1862) discovered the life history of *Taenia saginata*, the human beef tapeworm. He fed eggs to calves and recovered the larvae, *Cysticercus bovis*. Other similar life cycles were experimentally demonstrated, but the next significant advance was made when Grassi

and Rovelli (1889, 1892) showed that the larvae of the dog tapeworm, *Dipylidium caninum*, develop in the dog flea, and that they are not bladder-worms, but minute, solid-bodied forms with a small and transitory vesicle for which the term "cysticercoid" was used by Leuckart. In this case, the tapeworm eggs are eaten by the larvae of the flea. Grassi and Rovelli also showed that in *Hymenolepis nana* var. *fraterna* of rats, when eggs are ingested by rats and mice, the onchosphere larvae emerge, bore into the wall of the intestine, and complete their larval development there. When they have attained the cysticercoid stage, which is infective for the final host, they return to the lumen of the intestine, attach themselves, and grow into the sexually mature cestodes. Here, no intermediate host is necessary and the cestode can complete its life cycle in a single host species. More recently, Bacigalupo (1928, 1931) has shown that larvae of this rat tapeworm may develop in the body of the meal worm, *Tenebrio molitor*, and other insects, so the direct development in the intestinal wall of the rat or mouse probably represents an abbreviated type of life history in which the intermediate host has been dropped out of the cycle. This brief survey will serve as a background for the investigations which I wish to discuss tonight.

The strobilate stages of cestodes occur chiefly in carnivorous hosts, selachian and teleost fishes, amphibians, reptiles, birds and mammals, and these animals acquire the infection by eating other animals in which the larval stages are present. All of these vertebrates feed, either regularly or occasionally, on other animals, and even seed-eating birds supplement their diet with worms and insects.

It has long been known, however, that herbivorous animals, cattle, sheep, deer, horses, rhinoceroses, rabbits, monkeys, etc., are commonly and often heavily infected with adult cestodes. The worms are most abundant in the ungulates (hoofed animals), but are common also in rodents and marsupials. The tapeworms characteristic of herbivorous animals belong to a single zoological family, the Anoplocephalidae. The name is derived from *Anoplocephala perfoliata*, the giant tapeworm of the horse. A monograph of this family by Baer (1927) showed that at that time it contained 114 species, arranged in 28 genera and distributed as follows: 84 species in mammals, 17 species in birds, and 13 species in reptiles. They are common in domestic animals and, since sheep and cattle have been taken to all parts of the world by human migrations, their parasites have been taken with them and are cosmopolitan in distribution. These domestic animals, cattle, sheep and

horses, especially the younger individuals, may be seriously affected by these parasites. Several hundred specimens of *Moniezia expansa*, the common tapeworm of sheep, have been taken from a lamb 4–5 weeks old and, since the worms measure from 1 to 5 meters in length and up to 1.5 cm. in width, the intestine may be practically occluded by them. Another species parasitic in sheep, *Thysanosoma actinioides*, invades the liver and blocks the biliary passages. The pathology and symptomology of infection is recorded in standard textbooks of veterinary medicine. In view of the economic importance of these parasites, diligent search has been made for more than 75 years to determine the method by which they are acquired. The Bureau of Animal Industry in the United States Department of Agriculture has conducted experiments during practically all of this time. More than 50 years ago Curtice (1889, 1890), Stiles and Hassall (1893), and Stiles (1901) reported studies undertaken to discover the life history of the tapeworms of sheep and cattle. Similar studies were carried on in England, France, Germany, Russia, Italy, South Africa and Australia. Professor Leuckart, of Leipzig, regarded as *der Altmeister der Helminthologie*, and several of his students, devoted many years to the problem. One of these students, Riehm (1881), reported that the hares and rabbits of North Germany harbor five different species of anoplocephaline cestodes and, although extended observations and experiments were made, they failed to disclose the method of transmission. In the ensuing 60 years, a host of investigators has proposed all kinds of hypotheses, some fantastic, to explain the life cycle and method of transmission of these parasites. An examination of the literature covering this period discloses indefinite, contradictory and negative results. Opinion shifted from one point of view to another as repeated efforts failed to discover the life history. Many important observations were recorded, but few of the experiments were sufficiently well controlled to yield trustworthy results.

The repeated failures served to stimulate increasing interest in this baffling problem which came to be regarded as the great enigma of helminthology. It seemed preposterous and incomprehensible that every rabbit and sheep in a pasture could harbor tapeworms and that it was impossible to find out how they became infected. My work on this subject was begun in 1929. Since larger animals could not be used for laboratory studies, and since it would be difficult to control experiments on infection of such animals, I began with tapeworms of rabbits. Cestodes and eggs with their contained onchosphere larvae were ob-

tained from wild rabbits and the experiments conducted with domestic laboratory-raised specimens. Since there were a few reports of cestodes in domestic rabbits that had been allowed to range in the fields, it seemed probable that they could be infected in the laboratory. The food of rabbits can be carefully examined and such experiments conducted under conditions of rigid control.

Fortunately, sabbatical leave from New York University and a fellowship from the Guggenheim Foundation permitted a year (1931–1932) of uninterrupted study. The domestic rabbit was derived from the wild European species, *Oryctolagus cuniculus*, and is distinct from the wild American rabbit which belongs to the genus *Sylvilagus*. Furthermore, the tapeworms of the European wild rabbit are specifically distinct from those of the American wild rabbit. It seemed probable, therefore, that the domestic rabbit would be more susceptible to infection by tapeworms of the European rabbit than to those of the American wild rabbit. Riehm (1881), in the monograph cited earlier, had reported that the rabbits and hares of North Germany harbor five different species of tapeworms and so that region seemed the ideal one in which to conduct the experiments. The facilities of the Institut für Schiffs- und Tropenkrankheiten at Hamburg were generously placed at my disposal by the Director, the late Professor Doctor Fülleborn. Work was carried on there for the year. It was proved definitely that direct infection from one rabbit to another is impossible. Eggs containing living, moving larvae were fed to rabbits and were always recovered in the feces. They did not hatch and the larvae in them were alive and motile after passing through the digestive tract of a rabbit three or four times. Aging of the eggs and exposure to all kinds of environmental conditions did not affect the result. It was noted, however, that, if the shell of the egg was mechanically injured, the larva would emerge. These larvae measure approximately 0.02 mm. in diameter. Young tapeworms as small as 0.3 mm. in length were recovered from the intestine of naturally infected wild rabbits. The smallest ones consisted essentially of a small but perfectly formed scolex and it was clear that when first recognizable in the rabbit, the parasite had changed entirely from the onchosphere larva and had increased some 15 to 20 times in diameter. This growth and change of form must have occurred in an environment where protection and nourishment were available, that is, in the body of some intermediate

host. Epidemiological considerations made it possible to delineate that host with much clearness.

In the report of the work at Hamburg, I (Stunkard, 1934) made the following statement: "The intermediate hosts of anoplocephaline cestodes must be terrestrial invertebrates which in either larval or adult stages frequent the fecal material of the animals which harbor the sexually mature cestodes. Ordinarily, the eggs are very abundant in the feces and, if the intermediate host were to pick them up and feed on them, it would probably be unable to survive the severity of the infection. The ingestion of the onchospheres therefore is almost certainly accidental or incidental in the feeding habits of the intermediate host. It is possible that the intermediate host may acquire the parasites during only a portion of the year or during a certain period of its life cycle, but, once acquired, there is much evidence to indicate that this host will carry the infection for at least a year. The host is present and infective throughout the year, although much more abundant in summer than in winter. In view of the high incidence of infection and the large number of specimens that frequently occur in the intestine of a host (several hundred worms in the intestine of a lamb four weeks old), the intermediate host must be extremely abundant. It must be inconspicuous and very small (the intermediate host of *Andrya sp.*, e.g., may be accidentally eaten by a rabbit 2–3 weeks old). If, as believed, lambs become infected with *Moniezia* before they feed on vegetation, the intermediate host must not only be present on vegetation but actually enter the mouth of the animal. A large proportion of the definitive hosts feeds at night and the intermediate host may be a nocturnal species. Since the family Anoplocephalidae contains over one hundred known species, it may be expected that the intermediate hosts belong to a large family. Furthermore, this family must be world-wide in distribution and its members be more or less regularly eaten by herbivorous mammals, man, monkeys, cattle, horses, rhinoceroses, rodents, etc. These intermediate hosts are eaten also by birds and reptiles. The number of invertebrate animals which satisfy all of these requirements is small enough that the discovery of the life cycle may be confidently expected."

Since the onchospheres emerge from the eggs when the shells are mechanically injured, it seemed probable that the intermediate host was mandibulate or provided with a grinding stomach. Ants appeared to fulfill all the requirements and thousands of them were taken from

infected areas and dissected in the hope of finding the cysticercoid stage of the parasite. In addition, collections were made with a fine net, sweeping the vegetation in these infected areas at every hour of the day and night, and all of the animals thus taken were examined,—with only negative results. The year in Germany, although it provided much information, had not solved the problem.

The work was continued in New York and concentrated on the study of ants as possible intermediate hosts. I am indebted to Dr. W. S. Creighton of the City College for assistance in the collection, identification and housing of ants. The discovery by Jones and Horsfall (1935) that ants serve as intermediate hosts of chicken cestodes increased the suspicion that some ant transmitted the rabbit tapeworms. When dissection of ants collected in the field was fruitless, colonies of the common, smaller species were brought into the laboratory and fed the eggs of rabbit and sheep cestodes. But no infection resulted and it was apparent that the intermediate hosts were to be sought elsewhere. After six years of intensive effort, I was still groping in the darkness.

A fortunate combination of circumstances led to the discovery of the life cycle, and it was not the rabbit tapeworm, but the one from sheep, which yielded the first successes. When the experiments with ants were abandoned, the situation was desperate. It happened at that time that Doctors Ludwig and Fox of our laboratory, who had been studying Japanese beetle larvae, were annoyed by tyroglyphid mites that were overrunning their cultures. These mites had biting mouthparts and so eggs of both rabbit and sheep cestodes were fed to them. Dissection of the mites was begun several days later and, to my surprise and delight, larvae of both species of tapeworms were present in the body cavities of the mites. The eggs had hatched, the larvae had migrated to the haemocoele, but no development had occurred. The structure and bionomics of these mites precluded any hope that they were the long-sought intermediate hosts, but the preliminary success indicated that at last we were on the right trail. Many other kinds of free-living mites were collected from areas where sheep had not been allowed to graze and exposed to infection. The oribatid or beetle mites seemed to satisfy all the requirements postulated for the intermediate hosts and they proved susceptible. Eggs of both species of cestodes were fed to them, larvae were recovered from the body cavities, and, in members of the genus *Galumna*, the larvae of the sheep tapeworms

began to develop. Dissection of mites, at varying intervals after exposure, showed much development of the larvae, but, as a result of high mortality among the mites and the use of immature cestode eggs in some of the experiments, no mature cysticercoid larvae were obtained in the dissections of the first year. A group of 200 galumnid mites was fed eggs of *Moniezia expansa* and, after four months, they were fed to a lamb which had been kept for several months in our animal house. The lamb later passed cestode eggs in the feces, which showed that the mites had transmitted the infection, but the infective stage of the parasite had not yet been observed. In later experiments, the use of specially designed nests reduced mortality of the mites and care was taken to use only mature eggs for infection. During the summer of 1937, a series of developmental stages from the onchosphere to the fully formed cysticercoid larva was recovered from experimentally infected mites and the life cycle of this species was announced (Stunkard, 1937). A more complete account with photographs of selected stages of living larvae was published the following year (Stunkard, 1938). The results have been confirmed subsequently by the studies of Stoll (1938), Krull (1939) and Shorb (1939). Krull reported that the mites do not actually eat the eggs. Instead, they cut a hole in the shell and ingest the contents.

Meanwhile, six years had elapsed since the work in Germany, another sabbatical leave was at hand, and the support of the Oberlaender Trust made it possible to return to Hamburg and renew the study of the tapeworms of rabbits. The discovery of the life cycle of the cestode of sheep and the experience of the previous year in the "Tropeninstitut" served to orient the investigation. Wild rabbits are not readily obtained during the summer and early autumn but, when the hunting season opens, they are abundant in the markets. Accordingly, the early weeks were devoted to the collection and dissection of mites taken from areas where the infection was heavy. If cysticercoid larvae could be found and fed to young domestic rabbits, and if infection resulted, the cestode could be identified and its natural host would be known. Such information would be particularly valuable later when eggs of the cestodes were to be fed to possible intermediate hosts. Representatives of mites common in the Hamburg area were identified by Doctor Max Sellnick of Königsberg.

Over 6000 mites, representing 38 different species, were dissected between July 27 and November 1, 1938. On September 8th, dissection

of a specimen of *Scutovertex minutus,* an oribatid mite, yielded a fully formed cysticercoid which closely resembled those of the sheep tapeworms. It was 0.2 mm. in diameter and the scolex in it was alive and active. A small hole was bored with a needle in a grain of wheat, the larva was placed in it and the wheat was pushed down the throat of a half-grown, laboratory-raised, domestic rabbit. Examination of feces from the rabbit first disclosed cestode eggs on November 24th and they were abundant two days later. The animal was then killed and a single, mature, specimen of the rabbit cestode *Cittotaenia ctenoides* was found in the intestine (Stunkard, 1939). Eleven weeks had elapsed since the larva was fed to the rabbit. Other cysticercoids were found in the same and other species of oribatid mites and fed to laboratory rabbits, but no more sexually mature tapeworms were recovered. The other cysticercoids may have been immature or they may have belonged to species which do not develop in rabbits.

At this time, thousands of mites, representing more than 30 species, were collected from areas where they had no opportunity to become infected and they were kept in the laboratory for later infection experiments when eggs of rabbit cestodes would be available in large numbers. Meanwhile, many of the mites laid eggs, and young, laboratory-raised specimens were used in later experiments. During the autumn and winter, while rabbits and hares were plentiful in the markets, tapeworms and their eggs could be obtained at any time. Eggs of two species of cestodes, *Cittotaenia ctenoides* and *C. denticulata,* were fed to the mites taken from uninfected areas and to their laboratory-raised progeny. Successive stages in the development, from the onchosphere to the cysticercoid, were obtained in numbers. Development of *C. ctenoides* was observed in eleven species of mites and cysticercoids were recovered from seven of them. About ten weeks are required for the larvae to complete their development in the mite. Development of *C. denticulata* occurred in six species of mites and fully formed cysticercoids were recovered from *Scutovertex minutus.* The life cycle had thus been completed under controlled, experimental conditions. Successive stages were removed from the body cavities of the mites after they had been experimentally infected, and experimental demonstration has proved that the cysticercoids become sexually mature tapeworms in the intestine of domestic rabbits. Successive stages in the development of the worms in the final host were removed from naturally in-

fected wild rabbits. A detailed report (Stunkard, 1941), described the experiments and results of the study on the life cycle of rabbit cestodes.

The discovery of the life cycles of the tapeworms of sheep and of rabbits was supplemented by observations on yet another species. Although the anoplocephaline cestodes are found chiefly in the lower herbivorous animals, species have long been known to occur also in monkeys. Professor Studer of Bern, Switzerland, in 1879, found two of these worms in the intestine of a chimpanzee which had died in the zoological park at Lyon. One of the worms was studied by Professor Blanchard of the medical faculty of Paris, who (1891) named the species *Bertia studeri*. The name *Bertia* was preoccupied, so Stiles and Hassall (1902) renamed it *Bertiella*. When Blanchard described *B. studeri* from the chimpanzee, he declared that it was a potential human parasite and, twenty-two years later, he (1913) was the first to record it from man. The specimen was recovered from an eight-year old child at Port Louis, Mauritius. Other cases were reported from India and Africa, and Garcia (1935) described the parasite from a child, a monkey and a dog in the Philippine Islands. According to Joyeux and Baer (1929), *Bertiella studeri* is a common parasite of monkeys in Asia and Africa and these animals probably constitute the source of the infrequent human infections. Cram (1928) and Cameron (1929) reported human infections in the West Indies and it is clear that this tapeworm is an established human parasite in both the eastern and western hemispheres. There was no information concerning the life cycle and transmission of the monkey-human species until 1939, when I presented a report at the Third International Congress for Microbiology. The abstract of that report appeared in the proceedings of the Congress, and a longer account in the American Journal of Tropical Medicine (Stunkard, 1940). The study was made at the Institut in Hamburg. One of the monkeys in the animal house, a specimen of *Macacus rhesus*, originally from India, was passing tapeworm segments in the stools. Examination of the material showed that the proglottids and eggs belonged to an anoplocephaline species, probably *Bertiella*. Since free-living mites serve as intermediate hosts of sheep and rabbit tapeworms, it appeared probable that the monkey-human species had a similar life cycle. Accordingly, several hundred free-living mites, belonging to 24 different species, were collected from locations in the vicinity of Hamburg where they would not have been exposed to infection by tapeworms. Thousands of eggs were taken from the gravid

proglottids passed by the monkey and these eggs were fed to the mites. Dissections of mites were made at frequent intervals and showed that the mites ate the eggs, that the larvae emerged and migrated to the body cavities, where, in certain species, they followed a course of development practically identical with that of the sheep and rabbit cestodes. Fully formed cysticercoids were recovered from two species of mites and partial development was observed in others. The monkey was sacrificed and three specimens of *B. studeri* were taken from the intestine. It is clear, therefore, that the monkey-human species, like those of sheep and rabbits, utilizes free-living mites as intermediate hosts.

The results so far obtained have disclosed the life cycles of certain of the anoplocephaline cestodes of sheep, rabbits and monkeys, viz., *Moniezia expansa, Cittotaenia ctenoides, C. denticulata* and *Bertiella studeri*. Representatives of three genera have a common developmental pattern and all employ oribatid mites as intermediate hosts. It is probable, therefore, that related forms have a similar life history and a beginning has been made in the solution of the problems "How do the tapeworms of herbivorous animals complete their life cycles?" Only further studies can provide the final answer.

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WILLIAM A. RITCHIE, Rochester Museum of Arts and Sciences: *The Pre-Iroquoian Occupations of New York State.* (A. Cressy Morrison Prize Winner, 1943.)

The purpose of this work is to afford systematic expression to existing knowledge of aboriginal cultures in New York State, prior to the appearance, or at least to the dominion, of the Iroquois tribes, whose vanguard, it is believed, appeared in the wooded hills of our southeastern counties early in the fifteenth century of the Christian era. The data embodied in the correlation have been garnered over a period of many years by numerous individuals, lay and professional, to whom full credit is given in the appropriate pages. Principally, however, the reconstruction rests upon the investigations of the Rochester Museum of Arts and Sciences, conducted since 1924 when Dr. Arthur C. Parker became its director and the writer its archaeologist. Much of the material herein described has not previously been published, while that appearing in monographic studies has been re-evaluated and revised in the light of the whole before incorporation.

The surveys and excavations of the Rochester Museum have covered virtually the entire state except for its eastern portion; the knowledge so gained being supplemented by analyses of collections, private and institutional, situated throughout New York and its environs. While the present conception of culture configuration and sequence is not in any sense a fully formed imago, it is believed to be essentially sound, awaiting only further persistent efforts to develop and perfect its parts.

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The Distribution of the Salamanders of the Genus Plethodon in Eastern United States and Canada. (A. Cressy Morrison Prize Winner, 1943.)

Less than two decades have passed since the appearance of Dr. Dunn's (1926) excellent monograph on "The Salamanders of the Family Plethodontidae." Its inclusion of locality spot maps, showing the distribution of each form considered and a thorough discussion of their relationships, would indicate a certain degree of temerity on the part of the present writer in presenting an account of the distribution of *Plethodon*, one of the genera of this family. There is justification for this presentation, however. Much additional material has accumulated in museum collections since 1926. In this paper, for example, there are sixteen forms recognized in the eastern Plethodons where Dunn had but eight. One entire species group (Welleri), containing three species (*richmondi, nettingi,* and *welleri*), was not known when Dunn's book was published. Specimens have become available from regions that were comparatively inaccessible only a few years ago due, largely, to the expansion of the public highway system. Further, an attempt is made here to elucidate the facts of distribution by the consideration of applicable information from the recent literature of the cognate fields of glacial geology, physiography, phytogeography, genetics, and experimental embryology. Finally, it should be pointed out, that much new information about the genus under consideration has been made available since 1926 by Bailey, Bishop, Brimley, Dunn, Green, Heinze, King, Mittleman, Netting, Walker, and others.

This report is designed to be a study of geographic distribution. Those who are not familiar with the salamanders under discussion may read it with most profit if a copy of Dunn's book is at hand. It is considered beyond the scope of the present work to include morphological descriptions and an analysis of systematic characters; these are to be found in Dunn's book. However, where new forms are described, and where new groupings are proposed, a consideration of their systematic characters has been incorporated.

The original plan of this study was to survey the distribution of all eastern forms of the family Plethodontidae and this still remains

the over-all plan. Because of the uncertainty connected with existing conditions, however, it was thought best to present at this time such material as has been compiled. Other sections, on the remaining genera, may be looked for at varying intervals in the future. A treatment of *Desmognathus* and *Leurognathus* is now in preparation.

The present paper deals with the genus *Plethodon* in eastern United States and Canada. Certain western species of *Plethodon* are not considered to be within the scope of this report. These, listed in the chronological order of their descriptions are: *P. vehiculum* (Cooper), *P. vandykei* Van Denburgh, *P. elongatus* Van Denburgh, *P. dunni* Bishop, *P. idahoensis* Slater and Slipp, and *P. hardii* Taylor.

The eastern members of the genus may be divided into two distinct groups—one including the larger Plethodons with fewer costal grooves, and the other, the smaller Plethodons with more costal grooves. These two groups differ sufficiently from each other to be considered subgenera or even distinct genera. Allocations of this nature cannot be made, however, until the western members of *Plethodon* have been brought into the scheme; here the two groups will be referred to as the Large Plethodons and the Small Plethodons. The Large Plethodons contain two species groups, Metcalfi and Glutinosus, and the Small Plethodons, likewise, are comprised of two species groups, Cinereus and Welleri.